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Investigation of the Parallax of Groombridge 1830.
By M. Otto von Struve.

" I have just finished a series of observations and calculations on the parallax of *Groombridge* 1830, the star whose rapid proper motion was ascertained by Argelander in 1842. The method of observation followed by me in this research was that of measuring differences of declination between that star and two neighbouring stars; these I selected so that one of them (*b*) preceded *Gr.* 1830 about 3 minutes in time and 3′ to the north; the other (*a*) followed it by 2 minutes of time and about 0′·5 to the south: (*b*) is of the tenth, (*a*) of the eighth magnitude. The second star is that which was used by M. Faye in his determination of the parallax. My observations began in 1847, Nov. 4, near to one of the minima of parallax in declination, and were finished 1849, Dec. 2, including in that space two minima and two maxima.

" The common method of observing differences of declination, by equatoreals provided with wire-micrometers is well known. The operation begins by placing the wires in the direction of the daily motion, and observing the coincidence of the two wires, or the determination of the zero-point of the micrometer. Then the preceding star is brought to bisection by the fixed wire, by help of the micrometer-screw applied to the periphery of the declination-circle. This being done, it is supposed that the tube will not change its direction in declination between the transits of the two stars; and when the second star comes to the middle of the field, the moveable wire is brought, by help of the micrometer-screw, to bisect it.

" After the first day's observations, I remarked sometimes, especially in winter when the friction on the pivots is very heavy, that after having bisected the preceding star exactly, the instrument was a little changed in its position (not more, however, than some few tenths of a second) by a spring-like action of the arm, on which the declination-screw is directly applied. This action was immediately destroyed when, after having moved the tube with that screw in one direction, I gave it, at the end of the motion, a little turn in the contrary sense. But in proceeding in this manner I found great difficulty in obtaining the bisection of the star by the wire to the utmost visible exactness. To prevent this inconvenience, I had recourse to the following method of observation:—By help of the declination-screw I placed the star, in eastern horary angles, some tenths of a second (sometimes a little more or less) to the north of the wire, in western horary angles as much to the south, and observed the moment of the first good bisection, produced by the change of the refraction in different altitudes. To a horary angle of 5 hours corresponds, in our latitude, a zenith-distance of *Groombridge* 1830 of 50°, with a change of 7°·5 for each hour. At that altitude the change of the refraction is about 2″·4 for each degree of the altitude; therefore, having placed the star about half a second from the wire, I had to wait 1·6 minutes of time till the best bisection was produced by

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the change of refraction. This method of observation increases considerably the time employed, but at the same time it increases in a high degree the exactness of the results, and therefore I did not hesitate to follow this method for both stars in all cases where it was applicable.

" Near to the meridian this method of observation could not be used. Fortunately, in this position of the instrument, the motion of our refractor in declination is much smoother than in greater horary angles; therefore, in this case, I succeeded better in directing the wire on the star by the declination-screw; and in producing the bisection by turning the screw alternately in different senses, I was sure to avoid all constant errors. Nevertheless, I preferred the observations in greater horary angles; and in the later period of my observations I avoided, as much as I could, making them very near the meridian.

" I have mentioned already that I observed the moment of the first good bisection produced by the change of refraction. Owing to the trembling of the images, a good bisection will generally happen some seconds of time before the moment of exact bisection, that is, before the jumps of the star produced by the state of the atmosphere will be equal on both sides of the wire; but if I had waited for that so-called moment of best or exact bisection, I should have lost most probably a great many observations. For this reason I decided to observe the moment of first good bisection. The trembling of the images will be generally constant during two hours (the time employed for one evening's observation of *Groombridge* 1830), or at least it will change proportionally to the time. In both cases its influence will be eliminated in the difference of the two observed differences of declination, as soon as the observations are symmetrically arranged. On the contrary, in the sum of the two observed differences of declination, it will enter equally for both stars. As in our case, the preceding stars were both to the north of the following; in eastern horary angles the differences of declination observed by me ought to be a little smaller than those observed to the west of the meridian. This predicted phenomenon has been completely confirmed by experience. With only two or three exceptions, I have found the sum of the two observed $\Delta\delta$, or the differences of declination between the two stars of comparison, which ought to have been constant if they did not change their relative position in the heavens, constantly a little smaller at the east of the meridian than at the west, while, again, in their differences no trace of a constant difference is to be discovered. With a good harmony between the single determinations, the mean differences in the sum of the two $\Delta\delta$, measured on both sides of the meridian, is $0''\cdot31$; so that we have, for the mean effect of the trembling of the images on the bisection of one star, the value $= 0''\cdot08$.

" Another kind of error in the measured $\Delta\delta$ of two stars may arise, when, in the interval between the transits of both stars, the tube has changed its direction in declination by the effect of tem-

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perature, of more or less hygrometric state of the atmosphere, or for any other unknown reason. If such a change takes place, its action can generally be assumed to be proportional to the time elapsed between the two transits; and consequently, the difference of the two $\Delta\delta$ for a star situated in right ascension exactly in the middle between the two stars of comparison, would be quite free from that influence. In our case, the principal star was situated at a distance of $3^{m}\cdot1$ from the preceding, and of $2^{m}\cdot0$ from the following star of comparison; therefore, to avoid the errors produced by such a change, in the supposition of its proportionality to the time, we had to divide its total amount, on both observed $\Delta\delta$, in the proportion of 2 : 3. In some instances, there is no doubt, such a change has really taken place in our instrument; but its mean effect on the sum of the two $\Delta\delta$, or in the space of more than 5 minutes, deduced from the comparison of the single determinations with their mean, does not exceed $0''\cdot23$ for one day's observation. This number, as I have deduced it, ought to be found rather too great, as it includes all accidental error of observation. Its smallness, I think, is the best proof of the steadiness of our refractor.

"The exactness of measured distances, and likewise of measured $\Delta\delta$, with the wire micrometer, depends principally on the quality of the micrometer-screw. With the purpose of examining if the screw used by me was regular in all its parts, I brought it under a powerful microscope, and, having placed the wires of the microscope at a distance equal to 5 and 10 revolutions of the screw, I examined all different parts of the screw, and found them all so nearly equal one with another, that no measurable difference could be discovered in any part of the screw. Besides, to avoid more completely all possible error produced by an irregularity of the screw, or by an error in the division of the head of the screw, I never retained on two following nights the same zero-point, but continually changed it, so that all the different parts of the screw were used.

"The mean value of one revolution of the screw, and the influence of temperature on it, had previously been carefully determined. The method followed in this determination is explained in the *Description de l'Observatoire Central de Poulkova*, p. 193. The final value of one revolution of the screw was fixed there to be $r = 9''\cdot7319 - 0\cdot00022\, t$, where t signifies the temperature given in degrees of Réaumur. This determination of the influence of temperature on the value of our screw was, however, of no great importance for the determination of the parallax of *Groombridge* 1830, as the maxima and minima of the parallax in declination for that star occur in the months of April and October. The mean of the temperatures during the observations in the months adjacent to each epoch was $-1^{\circ}\cdot1$ R. for the maximum, and $-4^{\circ}\cdot8$ R. for the minimum of the parallax; so that the action of temperature on the determination of the parallax, if its whole coefficient had been neglected, would scarcely have produced an error of $0''\cdot01$ on the final value; therefore, if a small error should yet subsist in the

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indicated coefficient of temperature, its influence on the deduced parallax might be regarded as insensible.

"The coincidence of the wires, or the zero-point of the micrometer, was not subject to the least uncertainty. Every day it was determined 8 times, to make sure that no change had taken place in the position of the fixed wire. The method followed herein was to bring the moveable wire in contact with both sides of the fixed wire, and to take the mean of the two corresponding indications of the micrometer-screw. The single determinations got in this manner, and compared with their mean values, prove that the probable error of the mean, for one evening, is inferior to 0″·01.

"To the foregoing remarks on the methods of observation I might also add that every day, before I began a series of observations, the focus, as well of the object-glass as that of the eye-piece, was carefully adjusted to coincide with the plane of the wires. Perhaps in the first part of my observations I did not give the necessary attention to this point; and for this reason I have much more confidence in the later part of the observations, made since the autumn of 1848, than in the first part. Besides, it must be remembered that in this case, as well as in all particular inquiries of practical astronomy, where new methods of observation are used, for every observer some patience will be wanted before he can have acquired the necessary experience in the manipulation of his apparatus for that particular purpose, and before he will be quite satisfied with his observations.

"The power employed in this research was No. 2 of our refractor = 207 times. The difference of declination for the star preceding *Groombridge* 1830 being 3 minutes, a higher power could not be employed, if I wished to have both stars pass through the central part of the field, where, from the construction of our eye-piece, the images are the best defined; but I feel sure that if, in any other analogous inquiry, higher powers were to be used, the exactness of the observations would still be increased.

"A regular complete set of observations for one evening contained 8 comparisons of *Groombridge* 1830 with each of the two stars. If we denote the observed differences of declination between *Groombridge* 1830 and the star *a* by Δ, those between *Groombridge* 1830 and *b* by Δ', the comparisons were arranged in the following manner:—At first I observed, with the micrometer-screw to the south, four Δ; then, in the same position of the screw, four Δ'. This being done, the position-circle was turned 180°, so that the micrometer-screw was brought to a position opposite to the first, and then the observations were continued in the inverse order, namely, at first four Δ', and finally four Δ. This symmetrical arrangement of the observations appeared to be almost necessary, to avoid all influence of periodical changes in the mean of the results got by both stars. The reversion of the position circle was an object of high importance, as thereby all individual errors in the manner of producing the bisection by the micrometer-screw were destroyed. It must be mentioned here,

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that in producing the bisection of the star by the wire, the last motion of the micrometer-screw was constantly given in the same positive sense.

" Before and after each set of four observations, as I have already stated, the zero-point was verified, to show that no change had happened in it. At first, a constant difference of about 0″·15 in the zero-point of the micrometer, when used in opposite positions of the screw, was ascertained. Afterwards I made the spring which regulates the motion of the micrometer-screw act a little more strongly, and from that time this little difference was quite destroyed.

" The total number of observations made for the determination of the parallax of *Groombridge* 1830 was about 60. From this number all those were excluded in which, by a change in the state of the atmosphere, the complete number of comparisons with both stars in the fixed order was not obtained. Besides, the first two days' observations were also excluded, as for these the method of observation was not yet quite fixed, though they agree perfectly well with the results of the adjacent observations. After these exclusions there remain 47 complete observations; these observations are so distributed that 14 belong to a negative, 33 to a positive sign of the coefficient of the parallax in declination. The greater number of the observations made at a positive sign of the parallax depends solely on the more favourable state of the atmosphere in the spring, compared with that of the autumn.

Before I enter upon the results of my research, I wish to state that the foregoing detailed methods of observation were all fixed before beginning, or in the first two days of the observations. Besides, I can fairly declare that the observations were made, through all the time, without the least preoccupation with regard to the parallax. Until I had finished the whole series of observations, I had calculated nothing about the results, and I avoided even to think whether the maximum or the minimum of the parallax was to happen in the spring or in the autumn.

After having cleared the observed $\Delta\delta$ of refraction, aberration, precession, and nutation, they were reduced to the mean epoch of my observations, 1848, Sept. 20. In this reduction I used the proper motion in declination $= -5''{\cdot}782$, deduced by M. Peters from the comparison of his own observations with those made by Lalande, Groombridge, Bessel, and Nicolai. Then the equations of condition were formed and twice resolved by the method of least squares, with regard to the following two conditions:—

1. That the observed little differences in the sum of the two $\Delta\delta$ were merely a consequence of the method of observing the first bisection on the place of the best bisection, and that the apparent irregularities in this sum could be attributed to accidental errors of observation.

" 2. That these differences were produced by little periodical changes in the direction of the tube, and consequently were pro-

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portional to the intervals between the transits of each compared couple of stars.

"Both conditions could not be well united together in one system of equations, as I found some difficulty in separating exactly the qualities of their influence, especially as I had not indicated in my journal on what days I had observed the bisection produced by refraction or by direct motion of the tube.

"The resolution of both systems of equations, I. and II., gave the following results :—

I.	The parallax of *Groombridge* 1830	= + 0″·005,	with a probable error	= 0″·033
	The proper motion in declination	= −5·768	,,	= 0·026
II.	The parallax of *Groombridge* 1830	= + 0·051,	with a probable error	= 0·028
	The proper motion in declination	= −5·757	,,	= 0·023

"The probable error of the result for the difference in declination between *Groombridge* 1830 and the middle of the two compared stars obtained by one evening's observations was found to be = 0″·078. This number, however, corresponds only to the later period of my observations. In the first period the probable error for the result of one evening is about 0″·12. According to this difference of the probable error in the resolution of the equations, the weight attributed to the observations of the first period was 0·4 of that belonging to the later period. It may be mentioned here that the use of different weights for the observations made in the different periods was of no consequence on the result for the parallax. The resolution of the equations I. and II., with the same weight for all observations, gave the parallax (I.) = + 0″·009, (II.) = + 0″·053, both values according with the preceding two results within 0″·004.

"Neither one nor the other of the preceding two results can be regarded as quite exact, but the most exact value of the parallax following from my observations must lie between them both. However, I am inclined to give the preference to the second result, as by the method followed in the formation of the equations of condition a somewhat lower weight was attributed to the comparisons with the preceding star. This appears to be quite according with the nature of the observations, as, from the faintness of the star *b*, the observations of it in the full illuminated field were very difficult when the state of the atmosphere was not quite favourable; and if I was obliged, for this reason, to illuminate the field less, the bisection of the stars by the micrometer-wires was less accurate. This difficulty was never felt in the observations of the star *a*.

"The mean of the preceding two results for the parallax is + 0″·028, with a probable error equal to that of each of the single results, or about 0″·030. Hence it is evident that the parallax of *Groombridge* 1830 is so small, that the determination of its real numerical value is nearly out of the reach of the most perfect appliances of modern astronomy. If we do not adopt 0″·028 as an exact value of the parallax (the probable error being greater than the deduced

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quantity), at least we can say that it is nearly certain that the real value of the parallax is considerably inferior to a tenth of a second. Therefore the parallax = + 1″·08, determined by M. Faye, can only be attributed to an imperfection in the method of observation adopted by that distinguished astronomer. Also the value of the parallax = + 0″·226, found by M. Peters, is certainly erroneous, but it approaches near to the reality, as he had indicated for this value a probable error of 0″·141.

"The proper motion in declination of *Groombridge* 1830, determined by me and compared with that deduced by M. Peters from the absolute declination observed in the space of 53 years, is, I think, also a favourable proof of the quality of my observations. Both values accord within the limits of their probable errors; and the value determined by me, though only derived from two years' observations, is even superior to that of M. Peters with regard to the probable errors."

On the Computation of Ephemerides of the Moon.

By Mr. S. M. Drach.

The object of the writer is to prepare a form of Lunar Tables, slightly different from any at present in use, of which the characteristics may be described as follows:—

In the symbolical theory of the moon, the moon's longitude for any arbitrary time is necessarily expressed by sines and cosines of simple multiples of the time; and this form is retained by Damoiseau in the construction of his Lunar Tables. But Bürg and Burckhardt conceived that the calculation of ephemerides might be facilitated by using, as arguments for some following inequalities, multiples of time corrected by some preceding inequalities. Mr. Drach proposes to extend this system by correcting the multiples of the time, not (as Bürg and Burckhardt have done) by applying to them the exact amount of the preceding inequalities, but by applying to them certain multiples of the preceding inequalities. And Mr. Drach proposes to effect the multiplication of the preceding inequalities by this artifice. Suppose the multiples of time to be expressed by one graduation of the circle (*e.g.* the centesimal), and suppose the first inequalities to be expressed by another graduation (*e.g.* the sexagesimal); then, if we add to the *centesimal* argument the *numerical* value of the sexagesimal inequalities, we are adding to the argument a quantity really smaller than the amount of those inequalities. If the multiples of time had been expressed by sexagesimal graduation, and the first inequalities by centesimal graduation, then the addition of the *numerical* value of those inequalities would increase the arguments by a quantity greater than the inequalities.

The success of such an artifice must depend entirely on the skill with which its details are worked out; and, therefore, nothing more can be done at present than to describe the general principle.